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Interactive comment on “Air-sea CO₂ fluxes in the Atlantic as measured during the FICARAM cruises” by X. A. Padin et al.

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Anonymous Referee #2 Received and published: 27 July 2009 Review of the manuscript submitted to Biogeosciences Title: Air-Sea CO₂ fluxes in the Atlantic as measured during the FICARAM cruises Author(s): X. A. Padin et al. MS No.: bg-2009-113 Decision: This manuscript is not acceptable for publication in its present form General comment: This paper presents new observations of sea surface pCO₂ (or fCO₂) in the Atlantic Ocean for the period 2000-2008 from cruises regularly conducted along long transects from Europe to Antarctica during spring and autumn. In the context of international pCO₂ data synthesis and global carbon budget estimates, this study represents very important complementary data and should be published. However, the manuscript is somehow presented like a cruise report; discussions and interpretations

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are lacking, especially regarding previous works in the same regions. It is not easy to know what new results/informations have been obtained from this impressive 8 years of data.

Other comments: 1) Abstract: line 8: Authors indicate: -The obtained spatial and temporal distributions of FCO₂ follow the generally expected patterns and annual trends.- However, the paper does not compare the new FCO₂ calculations with previous studies and words in the abstract suggest that FCO₂ distribution did not really changed over a decade. If this is correct for all regions investigated here, from north to south, this is an important result as this would mean that the ocean carbon sink is not varying, at least in this region. If this result is robust, authors should highlight this finding in the abstract. However, although the title of the MS calls for FCO₂ results, the abstract only recalls one number, the influence of the Amazon River. Manuscript has been profusely changed for clarity. The comparison with Takahashi's climatology and other references has been added to the manuscript.

2) Introduction: authors should recall previous studies that investigate seasonal and interannual variability of FCO₂ in the Atlantic ocean. Introduction should include recent observational analysis (e.g. Takahashi et al., 2009; Schuster et al, 2009 and reference herein) and inverse atmospheric methods (e.g. Transcomm). In the atlantic ocean, bordered by continents (large terrestrial source/sink variability) and where few long-term atmospheric CO₂ monitoring station exist, oceanic FCO₂ observations, as presented in this paper, would greatly help to constraint the global carbon budget. Authors should also indicate what are the current FCO₂ errors in the Atlantic Ocean. Is it 0.1 PgC/yr or 1 PgC/yr ? They should also specify what is not known concerning the processes that control FCO₂ variability. This would more clearly justify why such long-term oceanic project like FICARAM is important. We have change profusely the text in order to clarify our objectives and the findings of this article.

Our study is part of the international effort to achieve the community's goal of being able to constrain the regional fluxes to 0.2 PgC year⁻¹ as recommended in Bender et

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al. (2002). This requires measurements with measurement accuracy of 3-10 μatm (<http://www.carboncyclescience.gov>). The study of the internal consistence of the in situ measurements recorded by our system using alkalinity and pH measurements (Padin et al., 2008) using Lueker et al. (2000) showed a consistence error of $\pm 6 \mu\text{atm}$ ($n = 365$; $r^2 = 0.91$).

3) Introduction page 5591 line 22: authors indicate that FICARAM data are available in several global databases. They should specify where the data are available. For example, using the Mercury search, FICARAM data are not available at CDIAC (see also the link http://cdiac.ornl.gov/oceans/VOS_Program/hesperides.html). The Introduction has been modified in order to include the suggestion. The new version of the manuscript is: "The surface CO₂ observations collected during FICARAM are part of databases compiled by the projects CARBOOCEAN and SOCAT (Surface Ocean CO₂ Atlas)."

4) Methods: pages 5593 and 5594: during FICARAM, atmospheric CO₂ has been regularly recorded (using air pump, DEKABON tube...). However, those data are not used for FCO₂ calculations. Instead authors preferred to use NOAA monitoring observations because during some FICARAM cruises atmospheric data are not available (why?). The manuscript has been modified including the explanation of because we use NOAA monitoring observations: "This dataset was preferred to in situ xCO₂atm data due to the lack of these records during the last five cruises on board the B/O Las Palmas. For consistency, measurements from the NOAA/ESRL were used in all cases."

It would have been interesting to compare FICARAM atmospheric xCO₂ data (when available) with those derived from NOAA atmospheric stations. Are the atmospheric data obtained onboard present or not significant deviations from continental and islands records. Are atmospheric xCO₂ data selected at monitoring stations have been filtered following air-mass trajectories to discard continental signal? If atmospheric data recorded onboard are not used, why authors describe these data in the methods section? The M&M has been rewritten in order to resolve these questions though we do not include description of in situ measurements and the comparison between these

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and the ones from NOAA atmospheric stations. In any case, the difference between both datasets is $0.0 \pm 8.5 \mu\text{atm}$ ($n = 33507$; $r^2 = 0.72$)

5) Methods: page 5595: authors convert $p\text{CO}_2$ to $f\text{CO}_2$, and then used Takahashi et al 1993 equation (but expressed for $p\text{CO}_2$, not $f\text{CO}_2$) to correct for temperature (SSTeq). This has certainly no impact on results but this is not recommended and should be revised. The equation has been included as originally described Takahashi et al. (1993).

6) Methods: page 5595. FCO_2 is calculated following equation (3), where “a” is a unit conversion factor. Could you please indicate what is the value used for “a” The parameter a is a unit conversion factor that has been removed from this Eq. 3 like most of the papers including the estimation of air-sea CO_2 fluxes. a is 0.0877 when you use $\text{mol m}^{-2} \text{yr}^{-1}$.

7) 2.4 Biogeochemical oceanographic provinces: authors separate different regions based on SST/SSS distribution and some known features such as upwelling, equatorial current, etc... I don't see where biogeochemistry is referred when you select the provinces? Change the title of section 2.4. The Introduction has been modified in order to include the suggestion. The new version of the manuscript is: “The study of the meridional distribution of the $f\text{CO}_{2\text{sw}}$ measurements in the Atlantic Ocean (excluding the Mediterranean basin) focuses on selected biogeochemical provinces established after Longhurst et al. (1995) and Hooker et al. (2000). Different sections of the FICARAM tracks were allocated in the appropriate following ten regions according to average boundaries established from SST–SSS relationships (Fig. 1)”

8) 3 Results and discussion: part 3 of the MS presents data and FCO_2 calculation, there is no discussion. Change the title of section 3. In spite of the manuscript shows a minor discussion, we have kept the name of this section.

9) Page 5601, line 19: need a reference when quoting upwelling system along Mauritanian coast. The text has been modified. The new version of the manuscript is:

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“...remotely sensed chl a observations that exceeded $-50 \mu\text{atm}$ (Fig. 3d) and 1.3 mg m^{-3} (data not shown) respectively were also observed along the Mauritanian coast (Wooster et al., 1976).”

10) Page 5601, line 22-23: Authors compare their results for autumn with annual flux from Takahashi et al. They should compare the results for the same seasons and using the same units. In this sense, the article has been modified in order to include the suggestion. So, the new Table 3 shows the comparison of the FICARAM measurements in the ocean waters with the values of the climatology of Takahashi et al. (2009).

11) Page 5601, line 26-27: Authors compare their seasonal results for the NEC (16N-8N) with annual flux from Takahashi et al over 14N-14S. They should compare the results for the same seasons and regions. By the way, are the FICARAM data included in Takahashi's pCO₂ synthesis. If yes, this is strange to obtain different results. If not, I strongly suggest authors to send their data in global databases (as it has been indicated by authors in the introduction, see comment 3 above). The FICARAM dataset was not included in the measurements used in the computation of Takahashi's climatology (Takahashi et al., 2009) because of we analyzed only two standard during the calibration and he demands at least three of them. Up to now, the measurements of FICARAM cruises are being flagged in the 2nd level quality control of SOCAT and the preliminary results pointing out the validity of this dataset.

12) Page 5603: line20-24: authors refered to several studies concerning the Livingston Island but those studies did not investigate this region. We agree with your comments. We have decided to modify the manuscript eliminating these references. The new version of the manuscript is: “Despite a $\Delta f\text{CO}_2$ value close to the air-sea equilibrium, high WS of up to 10 m s^{-1} through the “furious fifties” of the Southern Ocean resulted in relatively high CO₂ emissions to the atmosphere in the order $1.2 \pm 2.5 \text{ mol m}^{-2} \text{ yr}^{-1}$ (Table 2).”

13) Page 5605: authors refer to decreasing NAO index during the FICARAM experi-

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ment; however, in 2001-2008 the NAO index moved from negative to positive values. According to Osborn (2007), NAO winter index has declined from the early 1990s until 2005/2006. Moreover SST changes in the North Atlantic Ocean from 1990 to 2006 also showed the influence of this decline (Schuster et al., 2009), with a warming in northern latitudes, no change or a cooling in the mid-latitudes, and a warming in the tropical and eastern subtropical gyre. So, we are not agreeing with your comments.

14) Page 5606: authors indicate that increase of rainfall and riverine inputs from Amazon explain the observed decrease of SSS. Please add a reference. The manuscript has been changed. I have tried to include a significant date or reference showing growing discharges from Amazon River but I did not find anyone of them. In any case, I have consulted Prof. Nathalie Lefevre who deeply studied air-sea CO₂ exchanges of tropical waters in the Atlantic Ocean about it. She answered me she found similar results but it is quite complicated identify the forcing of this decline of SSS in the NECC waters.

The new version of the manuscript is: "The NECC showed a sustained interannual decrease in SSS (-0.16 ± 0.01 yr⁻¹) during the autumn season (Fig. 4d). The low SSS at these latitudes has been attributed to freshwater discharges from the Orinoco and Amazon rivers (Körtzinger, 2003; Ternon et al., 2000; Wanninkhof et al., 2007). The observed SSS drop coincided with a large $\Delta f\text{CO}_2$ reduction of -3.5 ± 0.9 $\mu\text{atm yr}^{-1}$, which increased the oceanic CO₂ uptake by -0.09 ± 0.03 mol m⁻² yr⁻². The continental inputs acted as fertilizers for the tropical surface waters of the NECC region, stimulating the biological drawdown of CO₂ (Lefevre et al., 1998; Subramanian et al., 2008)."

15) 3.4: could you explain why you are using an empirical algorithm to detect the forcing of fCO₂ variability. Empirical algorithm had been frequently used in order to reproduce the fCO_{2sw} variability and as extrapolation technique to different geographical scales that included geographical information, temperature, salinity, chlorophyll a, winter mixed layer in different polynomials (Olsen et al., 2004; Wanninkhof et al., 2007; Lueger et al., 2008; Padin et al., 2008; Padin et al., 2009). Furthermore the development of empirical and mechanistically based numerical methods for extrapo-

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lating in situ pCO₂ data and air-sea flux is recommended to model the upper ocean physics/biogeochemistry and sea surface pCO₂ (Bender et al., 2002).

16) Page 5607: could you justify the use of Lat/Long, SST and SSS second and third polynomials? What are the physical/biogeochemical justifications (meaning) of such selection in the diagnostic model. The algorithm is designed to explain the most part of the fCO₂sw variability beyond identify the processes governing these changes. Millero et al. (1998) fitted alkalinity distribution in several oceans using quadratic equation.

17) Page 5607: I understand you are first normalizing fCO₂ at constant SST to establish Equation (4). How this helped to investigate thermodynamic processes? The normalizing fCO₂sw at constant SST is done to remove the thermodynamic effect. So, the correlation SST – fCO₂sw would reflect in certain extend the pH variability of the water masses in relation to SST changes.

18) Page 5607: before establishing Eq 4, authors adjust fCO₂ data to a reference year 2005, i.e. they assume ocean CO₂ follows atmospheric trend but is it correct ? Recent studies suggest that oceanic fCO₂ growth rate is different depending the region and period. Is it realistic to apply the same correction in the Atlantic basin, from North to South? Why not including the period (year) in Equation 4 that may help to separate natural versus anthropogenic CO₂ signals. Considering the xCO₂atm changes in the different points we are including a particular interannual trend of xCO₂atm that is linearly interpolated from the one observed in the NOAA/ESRL Global Monitoring Division at each latitude of the Atlantic track. Thus, we are considering the spatial and temporal variability of the CO₂ increase in the atmosphere.

We assume fCO₂sw follows the long-term trend of CO₂ in the atmosphere and this is confirmed by the null correlation between the differences between the modelled fCO₂sw and in situ fCO₂sw that means, a similar trend. The exception was found in the CC region that anomalies showed significant changes of $0.60 \pm 0.05 \mu\text{atm yr}^{-1}$.

The following picture shows these residual for every region:

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19) Page 5608, line 14: authors indicate that in the southern hemisphere fCO₂ variability was poorly resolved with the model (their Eq 4) likely due to a larger coastal influence. However, on page 5598, authors specified that they have excluded data in coastal waters. Should you test another data selection for the southern hemisphere? The manuscript is refereed to the influence of continental inputs, upwelling events and shelf fronts. Such as it was pointed out on page 5598, we exclude data in proximal coastal waters that is located in a depth shallower than 50 meters. For clarity, the manuscript has been changed:

“Consequently, surface (<50 m) coastal waters that corresponded mainly to harbour areas were excluded from the database.”

“The fCO₂sw measurements obtained during the FICARAM cruises were modelled with an empirical algorithm according to their biogeochemical variability and geographical position (Olsen et al., 2004; Wanninkhof et al., 2007; Lueger et al., 2008; Padin et al., 2008; Padin et al., 2009) with emphasis on distinguishing ocean from distal shelf areas.”

20) Figures: figures 2 and 3 are very small; difficult to see all details. The Figures have been enlarged for clarity.

21) There are many references in the text that are not in the reference list: Cooper et al 1998; Richardson and Reverdin, 1987; Richardson and McKee, 1984; Klinck and Nowlin, 1986; Poisson et al., 1994; Pakhomov and Froneman, 1999; etc.... References has been changed

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Please also note the Supplement to this comment.

Interactive comment on *Biogeosciences Discuss.*, 6, 5589, 2009.

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